

Remarks on Directionality

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In several recent articles the issue of directionality in transformational grammar has been treated, rather unsatisfactorily to my mind. The question is this: Are the relationships among the various levels of grammatical description (semantic structure, deep structure, surface structure, phonetic structure) such that certain levels are descriptively prior to others? That is, is there an inherent "direction" to the relationship between two levels of description (say deep structure and surface structure)? Recent treatments suggest that the question is pointless, or that the answer is no. I maintain that this impression results entirely from the way previous discussions have been worded, and that the issues have yet to be approached properly.

The most important discussion, and one that deserves to be read with great care, is Noam Chomsky's in 'Deep Structure, Surface Structure, and Semantic Interpretation' (Chomsky, 1970):

Whereas the standard theory supposes that a syntactic structure E is mapped onto the pair (P, S) (P a phonetic and S a semantic representation), the new theory supposes that S is mapped onto E , which is then mapped onto P as in the standard theory. Clearly, when the matter is formulated in this way, there is no empirical difference between the "syntactically based" standard theory and the "semantically based" alternative. The standard theory generates quadruples (P, s, d, S) (P a phonetic representation, s a surface structure, d a deep structure, S a semantic representation). It is meaningless to ask whether it does so by "first" generating d , then mapping it onto S (on one side) and onto s and then P (on the other); or whether it "first" generates S (selecting it, however one wishes, from the universal set of semantic representations), and then maps it onto d , then s , then P ; or, for that matter, whether it "first" selects the pair (P, d) , which is then mapped onto the pair (s, S) ; etc. At this level of discussion, all of these alternatives are equivalent ways of talking about the same theory. There is no general notion "direction of a mapping" or "order of steps of generation" to which one can appeal in attempting to differentiate the "syntactically-based" standard theory from the "semantically-based" alternative, or

either from the "alternative view" which regards the pairing of surface and semantic interpretation as determined by the "independently selected" pairing of phonetic representation and deep structure, etc. Before one can seek to determine whether grammar is "syntactically-based" or "semantically-based" (or whether it is based on "independent choice" of paired phonetic representation and deep structure, etc.), one must first demonstrate that the alternatives are genuine and not merely variant ways of speaking in a loose and informal manner about the same system of grammar. This is not so easy or obvious a matter as is sometimes supposed in recent discussion.

Notice that the claim here is that 'at this level of discussion'--in the absence of any specific proposal for restricting the class of devices available for generating the quadruples (P, s, d, S) or for evaluating particular devices--no proposal to regard one or two of the types of representations as somehow more basic than the others can be empirically distinct from any other such proposal. That is, if the only empirical test of a device is its ability to generate a particular set, then clearly no two devices which generate the same set can be empirically distinct.

Consider the analogous problem in the field from which Chomsky's discussion springs, recursive function theory. The situation here is that there are many alternative ways of enumerating a given set. Take, for example, the set SQ of all pairs (A, B) where A is a whole number (1 or greater) in decimal notation and B is its square, also in decimal notation. Here is one way of enumerating the members of SQ :

- a. $(1, 1)$ is in SQ
- b. If (x, y) is in SQ , then $(x+1, y+2x+1)$ is in SQ .

where ' $x+1$ ' represents the result of adding 1 to x , and ' $y+2x+1$ ' represents the result of doubling x , adding this to y , and then adding 1 to this sum. In terms of this device, neither a number nor its square is 'more basic' than the other; the device derives the two in the same step. The pair $(4, 16)$ is derived by the following sequence of steps (in which the substeps in the additions and doublings are suppressed): $(1, 1)$, $(2, 4)$, $(3, 9)$, $(4, 16)$.

The enumerating device above is not, of course, the one that leaps first to mind. Instead, one thinks naturally of a device which treats A as basic, in the sense that the digits of B are derived by operations performed on the digits of A . This is the standard multiplication algorithm, applied to calculating the product of A and A .

Still another possibility would be to treat B as basic, and derive A by a square-root algorithm, or by some method of successive approximation.

Now if the ability of a device to generate a set is the only empirical test of its adequacy, there is no sense in which any one of the methods for enumerating SQ is better than any one of the others. What then is the source of the feeling that applying the standard multiplication algorithm is somehow the best, or simplest, or most natural way of enumerating SQ? Besides custom, there are several possible sources--that within some definitional framework, the set of squares can be defined on the basis of the set of whole numbers and certain fundamental operations, but not vice versa, or that given certain computational devices, the standard algorithm involves the fewest steps or the least amount of 'scratch space'. In both cases, some claims about psychological reality are necessary if the explanation is not to be vacuous, for otherwise we would be free to select any definitional system (in some of which the set of squares would be fundamental) and any type of computational device (in some of which it would be simpler to take square roots than to multiply). The definitional systems and types of computational devices must be assumed to embody, in part at least, significant claims about mental organization.

There has been an interesting development within recursive function and formal language theory, moving from studies about which functions can be computed and which cannot, to studies in the complexity of computation (so called 'time and tape' questions)--from questions of generative power to questions of simplicity, essentially.¹ At the same time, workers in several fields have considered many alternative axiomatizations of logic and number theory; here the goal has been some sort of systematic elegance, rather than psychological reality, although logical priority and psychological priority will often run together.

The point of this discussion is that it is possible to speak of a sense of directionality in recursive function theory, though the appropriate sense has scarcely been examined. This sense of directionality is relative to a descriptive framework, and the suitability of the framework must ultimately be determined by considerations of psychological reality.

The situation in linguistics is precisely analogous. It is time to move from questions of generative power to questions of simplicity within a descriptive framework, and to inform this investigation with considerations of psychological reality. The remarks of Chomsky's quoted above were directed principally against efforts to decide the question of directionality on grounds of a priori plausibility (see, e.g. Chafe, 1970, chapter 7). In this cause his remarks are entirely appropriate. They are appropriate directed against any tendencies to see grammars as models of production or comprehension, a position taken by stratificational grammarians, among others. However, these remarks do not even approach the question of directionality proper--whether an adequate descriptive framework for linguistics imposes, in part or in whole, a direction on the relationships among phrase markers, and if so, what direction is imposed in particular cases. These issues are totally obscured by the tone of Chomsky's presentation, which suggests that there cannot in principle be any issue of directionality. Unfortunately, others have responded to the tone of Chomsky's article rather than

to its actual content. Thus, Lakoff (to appear) writes:

the basic theory does not assume any notion of 'direction of mapping' from phonetics to semantics or semantics to phonetics. Some writers on transformational grammar have, however, used locutions that might mislead readers into believing that they assume some notion of directionality. For example, Chomsky (1970) remarks that '...properties of surface structure play a distinctive role in semantic interpretation'. However, as Chomsky points out a number of times in that work, the notion of directionality in a derivation is meaningless, so that Chomsky's locution must be taken as having the same significance as "Semantic representation plays a distinctive role in determining properties of surface structure" and nothing more. Both statements would have exactly as much significance as "Semantic representation and surface structure are related by a system of rules". The basic theory allows for a notion of transformational cycle in the sense of spects, so that a sequence of cyclical transformations applies "from the bottom up", first to the lowermost S's, then to the next highest, etc. We assume that the cyclical transformations start applying with P_k and finish applying (to the highest S) at P_1 , where k is less than 1. We will say in this case that the cycle applies 'upward toward the surface structure' (though, of course, we could just as well say that it applies 'downward toward the semantic representation', since directionality has no significance).

And Katz (to appear) argues, in essence, that there is no real issue between generative and interpretive semantics, because transformations and interpretive rules are merely inverses of each other; Chomsky's criticism of McCawley's (1968) treatment of respectively involves the same assumption, that to any transformation T there corresponds an inverse mapping (interpretive rule) T' , and vice versa.

One of the difficulties in discussing the issue of directionality is that it has been associated with various other issues, at least the following: lexicalism vs. transformationalism in the case of derived nominals, uniform vs. multiple lexical insertion, syntactic vs. semantic selectional restrictions, surface structure interpretive rules vs. derivational constraints, a distinction in kind between semantic rules and syntactic rules vs. the lack of such a distinction, the existence of a level of structure (underlying structure) serving as the natural base for syntactic rules but distinct from semantic representation vs. the identity of deep structure and semantic representation, a distinction in kind between semantic representation and syntactic representations vs. unity of representational systems.² Although it is an historical fact that one

group of investigators has inclined to one constellation of positions, while another set has adopted the opposed positions, there is in general no logical necessity for these groupings; lexicalism does not imply interpretivist semantics, nor does a belief in non-uniform lexical substitution commit one to derivational constraints, for example. An assortment of mixed positions is possible, and in favorable circumstances one can even imagine remaining uncommitted to a position on one question while energetically arguing another. But it is, unfortunately, not easy to argue on one fundamental assumption without adopting some position on others.

Another difficulty attending upon discussions of directionality is that the broad linguistic theory within which these discussions take place views a grammar as a device for enumerating n-tuples of representations at n linguistic levels (as in the Chomsky quotation above), without assigning any sort of reality to intermediate representations or to the rules relating representations at various levels. But it is the burden of work by Peters, together with Ritchie, that so long as a grammar is judged by its output (the set of n-tuples it generates), in comparison with the output of alternative grammars within some broad descriptive framework, there will be innumerable grammars adequate for any purpose; to distinguish alternatives, we must have either a rather narrow descriptive framework, or additional tests of descriptions, or both (cf. Peters, 1970, and Peters and Ritchie, 1969). Undoubtedly, part of the difficulty here arises from the fact that we have become accustomed to thinking of grammars as formal objects constructed with the aid of a set of general notational conventions. Very few take seriously the notion that the set of possible rules is very narrowly determined, that one might even consider listing them, preparatory to or in conjunction with a search for explanations of the list's membership.

It is instructive to compare theory with practice in generative grammar. Most of the actual work on the syntax of specific languages has assumed that there is a directionality in description, and has been concerned with the form of rules, the ordering of rules, and the content of remote representations. By and large, directionality has been an issue only to the extent that analysts have needed to determine which direction was to be associated with a particular mapping. These matters have been especially clear in phonology, where no one is inclined to be suspicious of the fact that the mapping (s,P) of systematic phonological representations to systematic phonetic representations is partitioned into a set of mappings called rules, and where the directionality of particular rules is uncontroversial. If phonology is ultimately to have any content, it will do so by virtue of the reality assigned to phonological representations and rules in themselves. The corresponding claim for syntactic rules has scarcely been defended--but see Bach ms. (1970) for an interesting treatment of interrogatives along these lines, complete with an assertion of universality for the rules he discusses.

To sum up: questions of directionality cannot be raised except in a reasonably narrow descriptive framework. Discussions at a high level (i.e. within certain broad 'theories of grammar') are

entirely pointless, because they cannot possibly decide any issue, just as discussion at a high level (i.e. within the broad theory of recursive functions) cannot possibly decide any issue having to do with directionality in cases like that of the set SQ mentioned above. Within certain frameworks the issue of directionality will be decidable. For example, in various restricted versions of the Aspects model, lacking both derivational constraints and surface structure interpretation rules, many sets of pairs (t_1, t_2) of syntactic representations t_1 and t_2 will have a clear direction associated with them, in the sense that the required mapping will be definable in one direction only; the infinite processes, or unbounded movements, treated by Ross (1967) will all be cases of this sort. In such frameworks, it may even be possible to define an overall directionality in syntax; what would be required is proof of directionality for particular rules, plus arguments for rule ordering, plus arguments for the absence of loops in ordering (other than those permitted by cyclical principles). However, these Aspects-based frameworks are known to be deficient in many ways, and very little effort has been spent on constraining the richer theories now under discussion. Hence, there is at the moment no credible framework available in which questions of directionality can be profitably raised. I look forward to theories embodying very strong substantive universals, theories in which these questions can be treated.

Footnotes

¹Compare the two chapters devoted to these topics in Hopcroft and Ullman (1969), already a standard text, with an earlier classic, Davis (1958), which does not even mention them. Even Minsky (1967) gives no treatment.

²There exists no careful and uncontentious treatment of these differences in fundamental assumptions. Postal (1970, Section V) is perhaps the best. Despite its title, Katz (1970) is not a balanced discussion of the two factions; instead, it is as much a polemic as Lakoff (to appear), and in addition is a prime example of the way in which many separate issues can be confused.

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